Clean Cars, Dirty Margins: Portfolio Responses to Electric Vehicle Subsidies*

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Abstract

Economists view Pigouvian pricing as central to efficient climate policy, yet transportation policy relies heavily on subsidies that misalign incentives across behavioral margins. We study the emissions consequences of Sweden's 2020 increase in plug-in hybrid vehicle (PHEV) subsidies. Using rich nation-wide administrative data and a fuzzy regression discontinuity design based on pre-determined lease-renewal timing, we estimate how exogenous subsidy exposure affects entire household vehicle portfolios and, importantly, driving behavior. Treated households choose lease-renewal vehicles that are 7.3 percent less fuel-intense, but retain vehicles that are 3.9 percent more fuel-intense, consistent with attribute substitution across cars. Driving responses offset as well: replacement vehicles are driven less, retained vehicles more, yielding a small but statistically insignificant decrease in total gasoline consumption. Implied carbon abatement costs average $1,061 per ton of CO_2 on a 100 percent clean electric grid, and we reject costs below $200 per ton at conventional levels. Accounting for within-household substitution substantially revises downward the direct environmental benefits of EV subsidies relative to standard assumptions.

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I. Introduction

Economists widely agree that Pigouvian prices are the linchpin of optimal climate policy, yet actual climate policy is typically comprised of subsidies, regulations and mandates. The main consequence of this distinction is that while Pigouvian prices align private and social incentives on most behavioral margins, implemented policies create an array of incentives that are misaligned with, and therefore likely to undermine, emissions reduction goals. Many climate policies operate through durable goods – for example, vehicles, appliances, industrial equipment – whose environmental consequences depend on both what is purchased and how intensively it is used. Also, in these settings, policy-induced changes in one part of a portfolio can reshape behavior elsewhere. A subsidy that encourages adoption of a cleaner technology may change what older capital is retained, how tasks are allocated across assets, and the intensity with which different assets are operated.

One critical sector that exhibits this distinction is transportation, where worldwide electric vehicle (EV) subsidies reached $41 billion in 2024 (International Energy Agency 2025). The incentives created by these subsidies differ from those of a carbon price since they do not raise the price of buying or driving conventional, higher emitting cars. Nonetheless, many models of decarbonization emphasize large emissions reductions from adoption of electric vehicles, typically through subsidies. These models often rely on undisciplined parameters of substitution in household vehicle portfolios and driving behavior (Knobloch et al. 2020; Keshavarzmohammadian et al. 2017).

A simple consumer choice model demonstrates that the optimal second-best subsidy depends not only on the level of unpriced externalities but, importantly, on the substitution patterns between clean and dirty goods (e.g. Sallee (2025)). In the EV context, relevant substitution margins arise from the fact that households typically own multiple cars. An accurate estimate of the environmental benefits generated by an EV subsidy must therefore account for several substitution margins. Does a subsidy-induced EV alter the composition of the vehicle portfolio? Is the EV driven as intensely as the car it displaced, and how does the choice affect the intensity of driving in other, potentially less energy efficient, cars in the household portfolio? Answers to these questions have eluded researchers for two main reasons. First, they require credibly exogenous variation in the level of EV subsidies; and, second, since such variation typically exists in household-level microdata, the researcher must observe household vehicle portfolio composition that includes time-varying usage for each car, which is rare.

While several papers estimate the effects of EV subsidies on vehicle choice (e.g., Xing et al. 2021; Muehlegger and Rapson 2022, 2023), and others examine EV usage patterns

after adoption (e.g., Davis 2019; Tal et al. 2019; Burlig et al. 2021; Chakraborty et al. 2022), this paper is the first to estimate how EV subsidies affect the composition of the entire household vehicle portfolio and the allocation of driving across all vehicles in that portfolio. Following Tebbe (2023), we exploit the pre-determined timing of fixed-term leases in a regression discontinuity design that creates quasi-experimental exposure to different EV subsidy regimes across otherwise similar households. Swedish administrative data allows us to observe lease status and renewal timing alongside vehicle attributes and annual kilometers traveled for every vehicle owned by a household. This enables us to quantify the extensive margin of vehicle choice, the retention margin within the household fleet, and the intensive margin of driving reallocation. Using this approach, we estimate all relevant margins of households' responses to an exogenous change in EV subsidies, and use the estimates to calculate the implied tons of carbon dioxide (CO_2) abated.

Sweden introduced a vehicle "feebate" system – which taxes high-polluting and subsidizes low-polluting cars – in July 2018 to accelerate the adoption low-emission vehicles. Our research design exploits a policy change in January 2020 that increased subsidies for plug-in hybrid electric vehicles (PHEVs). This policy change provides time-series variation in relative car prices that is, by itself, inadequate to support credible causal inference. However, we combine this variation with a key feature of the Swedish car market: a large share of people lease their vehicles and replace them on a fixed three-year cycle. Our identification strategy exploits the timing of, and constraints associated with, leasing contract renewals for households reaching the end of their three-year lease period just before or after the subsidy regime change.

To identify the causal effects of the policy, we employ a fuzzy regression discontinuity design that compares vehicle choice and driving behavior of households whose lease renewal occurred just after the increase in the PHEV subsidy (treated group) to those whose renewal took place just before the policy change (control group).[1] Assignment to treatment or control is therefore a function of decisions that the household made three years prior. A key advantage of this approach is that lessees could not have anticipated the policy change when initiating their lease, and deviations from the fixed lease schedule entail significant costs.

The paper presents several key results consistent with its main objective of obtaining a full accounting of the household vehicle emissions effects of adopting a subsidy-induced PHEV.[2] First, regarding vehicle choice, treated households select lease-replacement vehi-

1. We build on a literature that applies regression discontinuity designs in environmental and energy settings, including Greenstone and Gallagher (2008), Ito (2015), and others.

2. Note that this draft presents estimates of intent-to-treat effects, which we consider most relevant to policy. Future analysis may include estimates of local average treatment effects.

cles that are 7.3 percent *less* fuel-intense, on average, than those selected by control households. Vehicles that are retained by treated households are, on average, 3.9 percent *more* fuel-intense than those retained by control households. This result is consistent with "attribute substitution" across cars in a household vehicle portfolio, similar to the effect of fuel economy improvements documented in Archsmith et al. (2020). Overall, treatment causes a 3.0 percent decrease in fuel intensity of the household vehicle portfolio.

Second, a key advantage of our data is that we observe annual vehicle miles traveled for all cars in a household's portfolio. Using these unusually rich data, we find that lease-replacement cars in treated households are driven 2.7 percent *less* than lease-replacement cars in control households. Retained cars once again exhibit a countervailing effect. They are driven 4.1 percent *more* in treated households than control households. In total, household driving increases slightly, but not distinguishably from zero.

Next, we estimate the net effect on total household gasoline consumption. While it could be obtained by aggregating the extensive (vehicle choice) and intensive (driving) margin estimates just discussed, we estimate the effect directly after calculating gasoline consumption as the product of kilometers driven and fuel intensity (liters of gasoline per kilometer)[3]. The change in overall household gasoline consumption in treated households is indistinguishable from that in control households. This reflects a (significant) 8.1 percent *reduction* in gasoline from lease-replacement cars and a (significant) 6.9 percent *increase* from retained cars.

Finally, we convert gasoline consumption savings into CO_2 emissions to evaluate the cost-effectiveness of the subsidy program. Our point estimates imply that the cost of carbon abatement of this policy is \$1,061 per ton of CO_2, and we can reject costs of CO_2 abated below \$200 per ton with 95 percent confidence. These elevated abatement costs are especially striking since our calculations assume a carbon-free electricity grid, as is roughly the case in Sweden, and that the cost of CO_2 abated from EV subsidies on grids with higher emissions would be even higher. Our estimates are substantially higher than other estimates in the literature. For example, Cole et al. (2023) estimate the cost per ton of abatement from EV subsidies to be \$95 per ton in the U.S.

These results highlight how households respond as a unit to changes in relative vehicle prices, with important substitution margins occurring beyond the choice and usage of a subsidized vehicle. These factors have important implications for effective policy design in transportation decarbonization as they re-emphasize why first best policies (e.g. Pigouvian pricing via a carbon tax or permit trading regime) are so aspirational.

3. We use official fuel intensity ratings, although we acknowledge that true on-road fuel intensity may differ, as documented in Reynaert and Sallee (2021).

While our findings highlight how omission of certain margins of adjustment within household portfolios undermine the cost-effectiveness of subsidy-based approaches to transportation decarbonization, it is worth noting that several benefits and costs of EVs, and even EV subsidies, fall outside the scope of this paper. We do not investigate questions relating to EV charging infrastructure (Li 2016; Springel 2021; Dorsey et al. 2025), electricity pricing (Holland and Mansur 2008; Ito et al. 2018; Borenstein and Bushnell 2022), or the marginal emissions arising from electricity generation (Graff Zivin et al. 2014; Archsmith et al. 2015; Holland et al. 2016). Furthermore, recent work has shown that EVs can contribute positively to grid operations as a distributed energy resource (Bailey et al. 2025; Metcalfe et al. 2026; La Nauze et al. 2024). On the other hand, EVs charged at home using high voltage chargers (e.g. Level 2) may impose external costs on distribution infrastructure, raising questions about the prospects for managed control (Rapson and Shaffer 2025; Burlig et al. 2026) or distribution-level pricing strategies (Turk et al. 2024). Finally, subsidies may be suitable to address market failures arising from research, development, and investment externalities, as suggested by Acemoglu et al. (2012) and Langer and Lemoine (2022). While important, these topics fall outside the scope of our inquiry.

Our findings are relevant to a broad set of jurisdictions that have adopted a subsidy-forward approach to transportation decarbonization. Sweden exhibits car-use patterns similar to other high-income Western European countries (Focas and Christidis 2017). In the U.S., research reveals substitution across vehicle attributes in household portfolios in California, suggesting that our findings are likely relevant to multi-car households in general (Archsmith et al. 2020). The basic logic in this paper also extends beyond transportation policy. Subsidies for energy-efficient appliances may affect heating intensity or retention of older capital; education subsidies may reallocate effort or enrollment across institutions, including online learning; agricultural input subsidies may shift crop mix or land use. In each case, equilibrium adjustments within the portfolio determine policy effectiveness.

The remainder of the paper proceeds as follows. Section II discusses vehicle policies, the Swedish leasing market, and our data. Section III presents our empirical strategy and potential threats to identification. Section IV illustrates our main empirical results, and Section V derives implications of the policy. The final Section VI offers concluding remarks.

II. Background & Data

Our empirical strategy exploits a change in vehicle rebates that differentially affected households reaching the end of a standardized three-year lease contract immediately before versus after the policy reform. In 2020, 43 percent of privately registered cars in Sweden were leased rather than purchased. In this section, we first describe the Swedish feebate system and the timing of relevant policy changes. We then introduce the administrative data used in the analysis, describe the construction of the lease-renewer sample, and present descriptive statistics for the households and vehicles in our study.

A. Swedish feebate system

Figure 1 illustrates the timeline of Sweden's vehicle feebate system. The policy was introduced in July 2018, combining purchase rebates for low-emission vehicles with higher annual road taxes for high-emission cars. In January 2020, the policy was amended to increase subsidies for plug-in hybrid electric vehicles (PHEVs). A second reform followed in April 2021, which further increased rebates for BEVs while reducing subsidies for PHEVs and raising road taxes, thereby shifting incentives toward fully electric vehicles. In this paper, we focus on the effects of the first policy change that increased subsidies for plug-in hybrid electric vehicles, and which offers the cleanest variation.

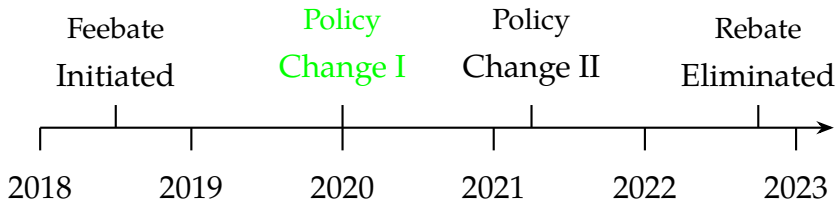


Figure 1: Timeline of vehicle feebate policy changes

Sweden's feebate system was designed to encourage the adoption of low-emission vehicles and deter the use of higher-emission ones. Under this system, financial incentives are provided for vehicles with lower emissions, while increased annual road taxes are imposed on those with higher emissions. The Swedish government introduced this policy through an ordinance issued in December 2017, which included the EV subsidies[4]. The amendment, effective from July 2018, applies exclusively to new vehicles registered in the Road Traffic Register from that date onward (Ministry of the Environment 2017).

Between July 2018 and December 2019, the vehicle rebate applied to vehicles that emit

4. We will refer to "subsidies" and "rebates" interchangeably throughout the paper.

a maximum of 60 g/km of CO_2.[5] For battery electric cars (BEV) with zero tailpipe emissions, the highest possible subsidy amounts to 60,000 SEK (≈ USD 6,312).[6] The bonus is reduced by a multiplication factor of 833 SEK (≈ USD 88) for every gram of CO_2 emitted per kilometer. The vehicle rebate for these vehicles is calculated using the formula: *Rebate* = 60,000 - 833 × C, where C represents the grams of carbon emissions per kilometer emitted by the vehicle.[7] The minimum bonus of 10,000 SEK applied to vehicles emitting 60 g/km of CO_2, with no bonus granted to vehicles exceeding this threshold. Figure 2 displays the vehicle rebate as a function of the vehicle emission level.

In January 2020, the Swedish government introduced modifications to its vehicle rebate system, which primarily impacted PHEVs, altering both the eligibility criteria for vehicle models and the corresponding bonus amounts. First, the CO_2 limit for new registrations to qualify for a climate bonus was increased to 70 g/km, thereby making more cars eligible for rebates. Second, the multiplication factor was reduced from 833 to 714 grams of CO_2 emitted per kilometer, resulting in an increased vehicle rebate for all PHEVs emitting between 1 and 60 grams of CO_2 per kilometer. Importantly, the annual road taxes remained unchanged in 2020, ensuring that there was no differential impact on the adoption of fossil fuel vehicles. Our identification will exploit the timing of lease renewals along with the fact that PHEVs received different vehicle rebates after this policy change under the assumption that the policy change was orthogonal to lease-renewal timing.

5. The bonus is granted no earlier than six months after the purchase to prevent the resale of vehicles outside the country.

6. We convert SEK to US dollars using the exchange rate from January 1, 2020 ($0.1052/SEK).

7. The bonus is capped at 25 percent of the vehicle's new price when purchased by a household. For example, when adopting a PHEV, the price should be compared to that of the most similar internal combustion engine (ICE) model. CNG cars receive a bonus of 10,000 SEK (≈ USD 1,052), independent of the CO_2 vehicle emissions.

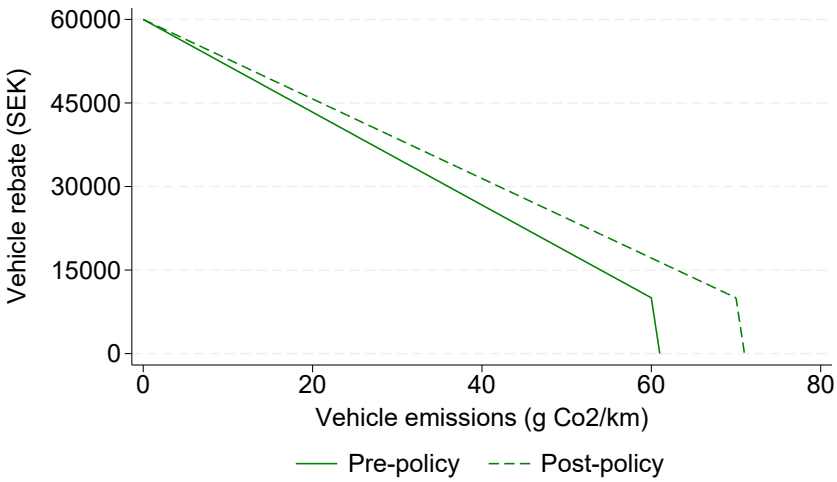


Figure 2: Rebate by vehicle emissions

Notes: This figure illustrates the vehicle rebate (SEK) as a function of the vehicle emissions for newly registered vehicles before the subsidy change (green dashed line) and newly registered vehicles after the subsidy change (green solid line).

B. Data sources

We combine data from several administrative sources provided by Statistics Sweden. These include the Swedish vehicle register (*Fordonsregistret*), the longitudinal integrated database for health insurance and labor market studies (*LISA*), and the geographic database (*Geografidatabasen*) from 2016 to 2022.

Vehicle data. The Swedish vehicle register contains information on all vehicle ownership and purchase records for the entire population of Sweden. The dataset includes details on the car's general status (registration date, owner type, whether it is leased, when the vehicle became the property of the current owner, and whether it is in use), the vehicle specifications (make, model, and trim), and various vehicle characteristics (service weight, fuel type, fuel efficiency, particle filter, carbon emissions).

Importantly, the register is a panel dataset that includes the annual vehicle kilometers traveled for each vehicle. Appendix B describes how the vehicle usage data is recorded in Sweden. For PHEVs, as we cannot observe whether the vehicle was driven on the electric or fossil drivetrain, we use the Worldwide Harmonized Light Vehicles Test Procedure (WLTP) standard for fuel consumption and adjust this value based on the share of kilometers driven using the fossil drivetrain from real-world usage patterns. Appendix B.2 describes the adjustment we apply to WLTP-based utility factors to account for real-world

PHEV electric driving behavior.[8] For BEVs, gasoline demand is zero.[9]

Additionally, each registration records a unique vehicle identification number and a social security number equivalent, which uniquely identifies all individuals in Sweden. For our analysis, we restrict the dataset to privately owned passenger vehicles (*personbil klass 1*) and exclude those registered for commercial purposes.

Demographic characteristics. To match individuals to their vehicles, we use individual identification numbers to link the Swedish vehicle registry to the LISA data, which contains socio-demographic information for Swedes over the age of 18, including gender, age, family status, income, gross salary, education, and employment status. To attribute commuting distances to each individual, we merge in data on employers and home locations via the geographic database.

C. Sample construction

Identifying the causal effects of EV subsidies on vehicle adoption and driving behavior requires changes in financial incentives that are orthogonal to unobserved preferences for vehicle choice and transportation demand. Comparing vehicle adoption and usage across households is challenging when replacement timing is endogenous and when households can freely adjust the timing of vehicle purchases in response to policy changes. To address these concerns, we focus on a sample in which vehicle replacement decisions occur at predetermined points in time and are therefore plausibly exogenous with respect to contemporaneous policy changes.

Our empirical analysis centers on households who initiated a vehicle lease between January 2016 and December 2017, and who subsequently reached the contractual renewal point approximately three years later. We exploit a key feature of the Swedish vehicle market that allow us to identify shifts in the adoption of new vehicles. Lease agreements in Sweden are typically written for exactly three years (1,095 days), generating a sharp concentration of vehicle replacement decisions at this horizon (Tebbe 2023). Figure 3 plots the probability of adopting a new vehicle as a function of days elapsed in the current lease contract. The figure reveals a pronounced spike in vehicle replacement at the three-year mark, indicating that leaseholders renew at or near the scheduled expiration date. We restrict the sample to leaseholders who renew their vehicle within a narrow window

8. We assume households with PHEVs drive on electricity only for a subset of total miles, with longer electric-only ranges corresponding to lower reliance on gasoline (Isenstadt et al. 2022).

9. For the CO_2 calculations, we assume zero emissions from the electric portion of PHEV use, focusing only on end-of-pipe emissions and excluding upstream emissions from electricity generation; given Sweden's predominantly renewable electricity mix, marginal emissions from vehicle charging are effectively zero (Morfeldt et al. 2021).

around the scheduled three-year expiration date, allowing for a ± 30 day deviation from the contractual renewal date, as indicated by the dashed lines in Figure 3. This restriction isolates leaseholders who are most directly exposed to the policy change and minimizes confounding variation arising from early terminations or lease extensions. To map individual renewal decisions to household-level outcomes, we further restrict the sample to families with exactly one individual at the three-year lease renewal, which excludes approximately 0.4 percent of the sample.

Treatment status is determined by the timing of lease expiration relative to the January 2020 subsidy reform. Households whose leases expired in 2019 serve as the control group, as they renewed prior to the increase in PHEV subsidies, while households whose leases expired in 2020 form the treated group and were eligible for the higher subsidy at renewal.

By combining predetermined variation in the timing of vehicle replacement decisions with a discrete change in financial incentives for plug-in hybrid electric vehicles, our empirical setting allows us to compare vehicle adoption and subsequent driving behavior across otherwise similar households. We exploit this variation to identify the causal effects of subsidy-induced PHEV adoption on household gasoline consumption, accounting for behavioral adjustments across the entire vehicle portfolio.

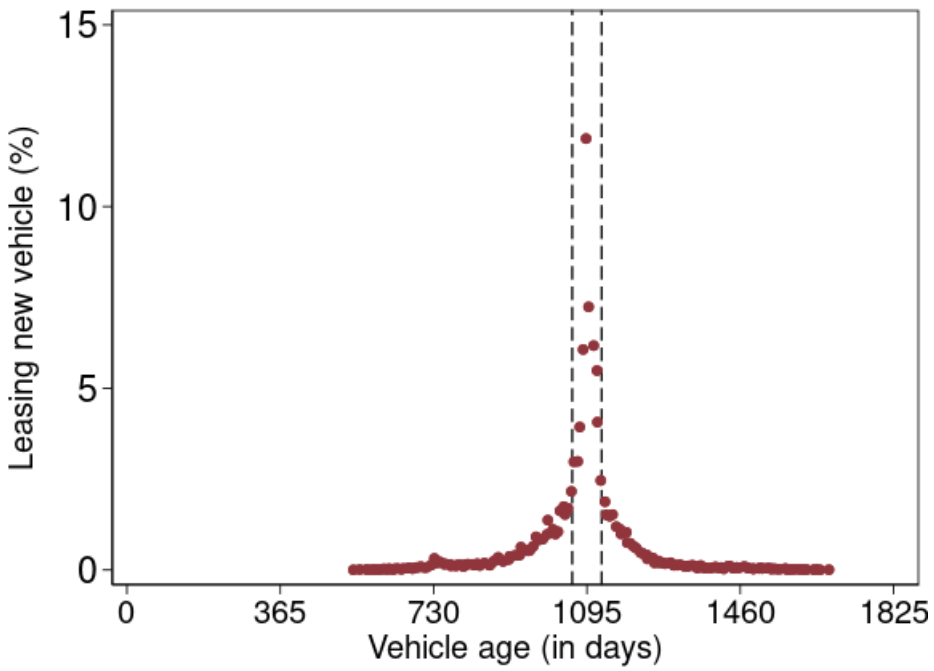


Figure 3: Probability of adopting a new vehicle by days in lease contract

Notes: The figure illustrates the contract renewal probability of leased cars with respect to the time in the current car contract (i.e., days since first registration). Dashed vertical lines reflect sample restrictions that we impose.

D. Descriptive statistics

The descriptive statistics demonstrate that our sample reflects the broader population of Sweden. Table A1 reports summary statistics for socio-demographic characteristics (Panel A) and vehicle attributes (Panel B) for households in our study sample, new car adopters, and the broader population of vehicle owners in Sweden. Households in our sample are slightly younger than new car adopters, but older than the average vehicle owner. They have somewhat lower incomes and fewer years of education than new car adopters, while remaining broadly comparable to the overall population. The share of women and household characteristics such as marital status and children are similar across groups.

In terms of vehicle characteristics, cars in our sample are older, driven more intensively, and have higher fuel consumption and emissions than vehicles owned by the average new car adopter, but they closely resemble the average vehicle in the Swedish fleet. Consistent with this, gasoline consumption and vehicle weight in our sample lie between those of new car adopters—who operate newer, cleaner, and lighter vehicles—and the average vehicle owner. Overall, the table indicates that while our sample differs from new car adopters along expected dimensions, it remains broadly representative of the wider population of vehicle owners, supporting the external validity of our analysis.

Table A2 reports average characteristics of newly registered vehicles for the treated group, the counterfactual group, and the full population of new vehicles. Vehicles purchased by treated households are, on average, cleaner and more fuel-efficient than those purchased by the counterfactual group, as reflected in lower fuel intensity and carbon intensity. At the same time, vehicles purchased by the counterfactual group are already cleaner than the average new vehicle in the same period. This suggests that households approaching lease renewal may have a tendency to select relatively clean vehicles even in the absence of subsidies, implying that baseline vehicle choice among this group is already cleaner than for the average Swedish car owner.

III. Empirical Strategy

A. Outcomes of interest

Our primary outcome of interest is household gasoline consumption $\text{Gas}_{i,t}$, which we construct by aggregating fuel use across all vehicles owned by household i in year t. Specifically, gasoline consumption is constructed as

$$\text{Gas}_{i,t} = \sum_{j=1}^{N_{\text{cars},i,t}} \text{Fuel intensity}_{j,i,t} \times \text{Vehicle kilometers}_{j,i,t}, \tag{1}$$

where fuel intensity and vehicle kilometers traveled are measured at the vehicle level and summed over the household's entire vehicle portfolio. This construction allows us to capture not only changes in fuel intensity arising from the adoption of cleaner vehicles, but also behavioral responses through changes in driving. By decomposing gasoline consumption into fuel intensity and kilometers traveled, we distinguish between reductions driven by cleaner cars and offsetting responses arising from reallocation of driving within the household.

We can also decompose household vehicle outcomes by vehicle type, distinguishing between newly leased vehicles and retained vehicles. New cars are all cars that were adopted after 2019, while Retained cars are vehicles that were not newly leased and remain in the household vehicle fleet. For new vehicles, changes in gasoline use arise through two channels: first, changes in fuel intensity driven by substitution from fossil-fuel vehicles to plug-in hybrid electric vehicles (PHEVs), as well as substitution within the set of subsidized PHEVs across treated and control households; and second, changes in driving behavior following lease renewal. For retained vehicles, gasoline use changes reflect differences in driving behavior and, differences in the fuel intensity of vehicles exiting the fleet. Appendix D.2 presents a decomposition of gasoline demand from new and retained vehicles.

B. Research Design

Estimating the causal effect of PHEV adoption on gasoline consumption faces several empirical challenges. First, subsidy availability and vehicle adoption are not randomly assigned across households. Households that choose to adopt subsidized PHEVs likely differ systematically from non-adopters in unobserved preferences for environmental goods, driving patterns, or willingness to adopt new technologies. These selection effects could bias cross-sectional comparisons if, for example, early PHEV adopters would have reduced their gasoline consumption even in the absence of the subsidy. Second, vehicle choice is inherently correlated with transportation demand: households anticipating higher future driving may select different vehicles than those expecting low vehicle usage, making it difficult to separate the effect of vehicle choice from underlying demand differences. Third, data limitations compound these challenges. Measuring gasoline displacement requires observing both individual vehicle kilometers traveled and the complete household vehicle portfolio over time, as substitution effects across vehicles within multi-car households can substantially offset reductions from newly adopted PHEVs. These data are rarely available in conventional datasets, which typically observe either aggregate household fuel purchases or individual vehicle attributes, but not both at the household-vehicle-year level

necessary to identify behavioral responses.

We overcome these challenges by using comprehensive nation-wide Swedish administrative vehicle data on all vehicles in a household along with a fuzzy regression discontinuity design (RD) design to estimate the extent to which a newly acquired PHEV displaces gasoline demand. The discontinuity arises from changes in vehicle subsidies for PHEVs among households with expiring leasing contracts. Lessees whose contracts expired after January 2020 were eligible for extended vehicle subsidies, whereas those whose contracts expired before this cutoff were offered lower subsidies.

The RD design allows us to estimate the effect of the subsidy on both new vehicle choice as well and usage of new and retained cars. Specifically we estimate the following equation on outcome of interest y_{it}:[10]

$$y_{it} = \alpha + 1[t \geq c](g_l(t-c) + \lambda) + 1[t < c]g_r(c-t) + \delta Z_i + e_{i,t}, \tag{2}$$

where c is the cut-off date for the vehicle rebate, t is the date of the leasing contract renewal, and g_l, and g_r are unknown functions. λ delivers the regression discontinuity estimate under the assumption that other policy changes and covariates are continuous at the threshold. We additionally control for covariates Z_i that may differ discontinuously across lease renewers on either side of the cutoff in the regression discontinuity design.

We exclude households with lease renewals three weeks before and after the policy change in January 2020, since households very close to the cutoff may have been able to strategically delay or accelerate renewal in response to the announced policy change. This "donut" restriction ensures that treatment assignment is driven by predetermined lease timing rather than contemporaneous vehicle choice.[11]

To credibly estimate the magnitude of the displacement effect, we assume that no other factors changed discontinuously at the cutoff date and that the sole channel through which fossil fuel kilometers are affected is the change in vehicle usage of newly subsidized PHEVs. Additionally, the validity of the 2SLS estimation relies on the monotonicity assumption, which requires that the vehicle rebate did not lead any lease renewers to decrease their PHEV usage.

Although our estimated treatment effects apply specifically to a population of leaseholders renewing their contracts after three years, we argue that this population is highly

10. See Imbens and Lemieux (2008) and Lee and Lee and Lemieux (2010) for details on the implementation and assessment of RD designs.

11. This approach is similar to (Dahl et al. 2014), who exclude births occurring immediately before and after a policy change to avoid potential strategic timing of births in response to announced reforms. They exclude a two-week window on each side of the policy cutoff to address concerns about strategic manipulation of birth timing.

relevant for predicting future EV adoption and usage patterns. Leasing is becoming an increasingly dominant mode of vehicle acquisition in many countries, particularly for EVs, which account for a growing share of newly leased cars. Moreover, the structure of leasing markets implies that subsidies are directly passed through to leasing rates.

Intuition. To conceptualize how our identification strategy captures the displacement effect, consider two similar households (illustrated by the red dashed circles) who initiated a new lease contract at the end of 2016 (bottom) and the beginning of 2017 (top) (Figure 4). Assume both households renew their lease contracts precisely at the 3-year renewal threshold. While the household at the bottom receives a lower subsidy for a PHEV upon lease expiration, the household at the top receives a higher subsidy for a PHEV. Consequently, the person at the top adopts a new PHEV after the policy change in January 2020, while the person at the bottom opts for a new fossil fuel vehicle. The identification strategy compares the subsequent driving behavior of the household with the new PHEV to that of the household who adopted a new fossil fuel car just before the cutoff. As a result, any differences in vehicle usage following the lease renewal provide insight into the displacement effect. The variation arises because the timing of the renewal determines whether households lease a new electric or fossil fuel car.

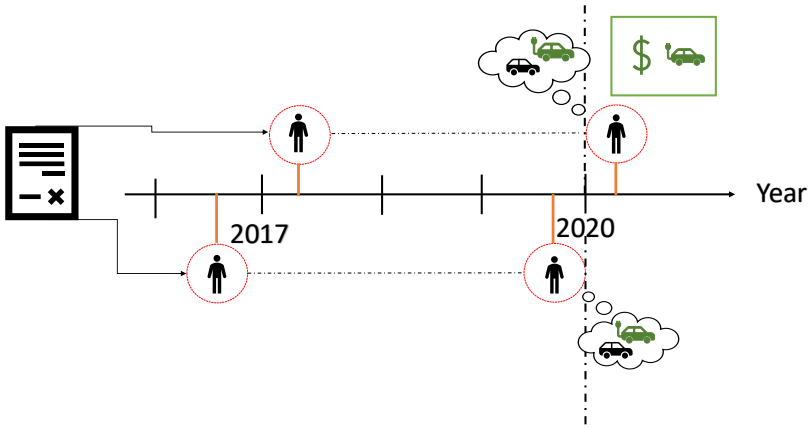


Figure 4: Identification intuition

Treatment timing. We define the change in household gasoline consumption (ΔGas_h) as the difference between post- and pre-period gasoline use. Formally, the main outcome of interest is given by

$$\Delta \text{Gas}_i = \Delta \text{Gas}_i^{\text{treated}} - \Delta \text{Gas}_i^{\text{control}} \tag{3}$$

$$= \left(\overline{\text{Gas}}_{i,\text{post}}^{\text{treated}} - \overline{\text{Gas}}_{i,\text{pre}}^{\text{treated}}\right) - \left(\overline{\text{Gas}}_{i,\text{post}}^{\text{control}} - \overline{\text{Gas}}_{i,\text{pre}}^{\text{control}}\right), \tag{4}$$

To implement the RD design, we compare gasoline consumption within the same calendar years but at different points in the vehicle life cycle, thereby holding fixed time-varying factors such as fuel prices, macroeconomic conditions, and nationwide vehicle policies. Figure 5 illustrates this *calendar-year comparison*. As shown in the figure, control households signed their initial lease in 2016, while treated households signed theirs in 2017. Both groups then renewed their leases approximately three years later—control households in 2019 and treated households in 2020. The key feature of this design is that we measure gasoline consumption in the same calendar years for both groups: 2018 serves as the pre-period (roughly one year after the initial lease for both groups), and 2021 serves as the post-period (roughly one year after lease renewal for both groups), as indicated by the shaded areas in the figure. By holding the calendar year fixed across groups, this design absorbs common time-varying shocks that affect all households equally, such as changes in fuel prices or aggregate economic conditions.

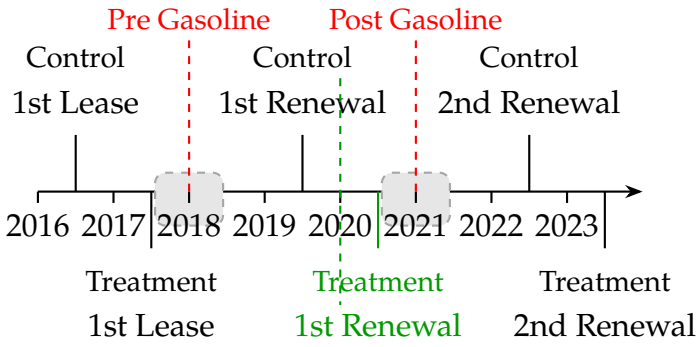


Figure 5: Calendar year comparison

In addition to the institutional features that lend credibility to our design, the timing of the subsidy change is unlikely to be confounded by COVID-19 mobility restrictions. First, unlike many other countries, Sweden did not impose legally enforced lockdowns or broad travel bans during the pandemic, instead relying on public health recommendations and limited social distancing measures; there were no nationwide stay-at-home orders or mandatory limits on travel comparable to the strict lockdowns seen elsewhere (Bohman et al. 2021). While mobility did decline during the early pandemic period, these changes appear to reflect voluntary behavioral responses rather than government-mandated travel

restrictions (Born et al. 2021). Second, by comparing households with lease expiration immediately before and after the subsidy change, we hold constant household-specific driving patterns and seasonal effects and preserve the core variation in lease renewal timing relative to the January 2020 subsidy change. As a result, variation in lease timing relative to the subsidy remains plausibly exogenous and likely unaffected by pandemic-related shocks. Finally, while we exploit lease renewals around the 2020 event, driving behavior and gasoline consumption comparisons are between 2018 and 2021. We thus exclude 2020 as a comparison year for these outcomes.

C. Identification tests

The validity of our RD design requires that households cannot manipulate the assignment variable, the timing of the lease renewal, near the cutoff of the policy change. There is little opportunity to strategically time the starting of the lease contract 3-years before in anticipation of policy changes, as the implementation date for the reform was announced just before the respective policy. However, it is still possible that leaser deviate from the three-year lease renewal timing and either renew earlier or renew later to receive the bonus system. In Figure 6, we document, however, that the share of lease renewals for both policy changes is smooth around the cutoff. In addition, as deviating from the lease contract is costly, we show that these deviations are typically more expensive than changes in vehicle policies. We also find that the average number of days between lease renewal and first lease initiation are similar for treated and control households (Figure C1).

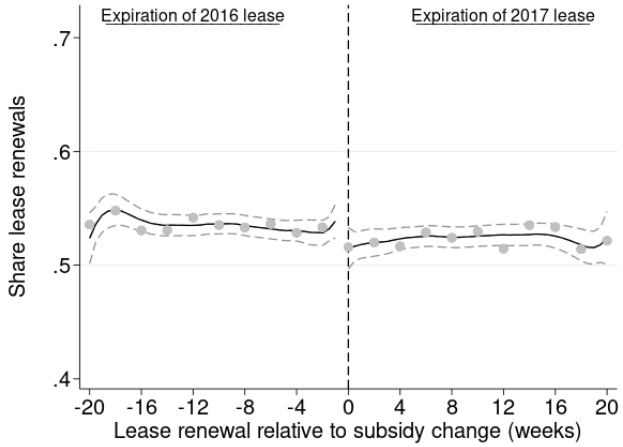


Figure 6: Lease renewal probability

Notes: The figure shows the share of households who adopt a new vehicle at the lease renewal around the 2020 policy change. Dashed vertical lines denote the subsidy change of January 2020 (normalized to 0).

Figure 7 presents the demographic characteristics of the leaseholders before and after

the renewal of the contract, relative to the introduction of the subsidy. The plots indicate that key characteristics such as age, income, employment status, and education remain balanced across both periods, suggesting that leaseholders renewing their contracts before and after the policy change are comparable in terms of demographics. This supports the assumption that any observed differences in outcomes are likely attributable to the subsidy rather than changes in the underlying population composition.

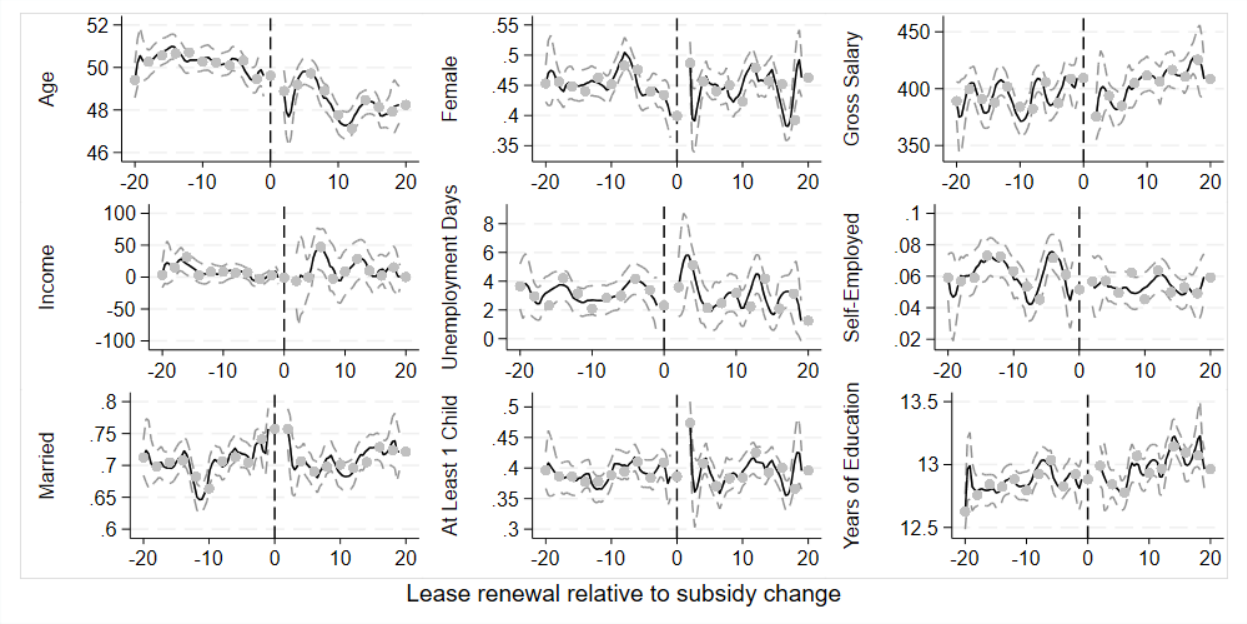


Figure 7: Demographic characteristics of leasers around policy

Notes: The figure shows nine demographic characteristics among individuals who adopt a new vehicle at the lease renewal around the 2020 policy change. Dashed vertical lines denote the subsidy change of January 2020 (normalized to 0).

Although our main analysis focuses on the 2018-2021 difference for main outcomes, we also verify that the pre-treatment (2018) distribution of vehicle ownership is comparable between control and treated households (Figure A1).

IV. Empirical Results

This section first quantifies the effect on household gasoline demand and its underlying components, then evaluates the cost of the feebate reform through changes in subsidy intensity and eligibility, and finally assesses the robustness of the results to alternative bandwidth choices. We characterize our results for three sets of vehicles:

- **New Cars:** These are all new cars adopted after January 2019 by households assigned to either the treatment or control group

- **Retained Cars:** These are all other vehicles in the household other than the new vehicle.
- **All Cars:** These are all cars in the household around the policy change window.

A. Gasoline demand

Table 1 reports regression discontinuity estimates of the effect of the subsidy-induced lease renewal on gasoline demand using the calendar-year comparison, decomposed into total household gasoline use (Column 1), gasoline use from new vehicles (Column 2), and gasoline use from retained vehicles (Column 3). Panel A shows that the subsidy leads to a statistically significant reduction in gasoline consumption from newly leased vehicles of approximately 85 liters per year (8.1 percent). This reduction reflects both substitution toward cleaner vehicles at lease renewal and lower gasoline consumption by these new vehicles. In contrast, gasoline consumption from retained vehicles increases by about 72 liters (6.9 percent), consistent with treated households reallocating driving toward retained fossil-fuel vehicles following the adoption of a subsidized PHEV. As a result, the net effect on total household gasoline consumption is a modest and statistically insignificant reduction of 13 liters per year.

Panels B and C provide insight into the underlying mechanisms. Panel B shows that treated households drive newly leased vehicles approximately 534 kilometers (2.7 percent) less per year, while driving of retained vehicles increases by 811 (4.1 percent) kilometers, contributing to the offsetting effects observed in total gasoline use. Table C2 documents the effect of the subsidy on vehicle usage by fuel type. We estimate that plug-in electric vehicles increase annual driving by 528 kilometers, while fossil fuel and hybrid vehicles show no statistically significant changes in usage. This represents a key parameter for our cost-abatement calculation, as the additional EV driving generates incremental electricity demand and associated emissions from charging.

Panel C indicates that fuel intensity declines significantly for newly leased vehicles by 0.39 liters per 100 kilometers (7.3 percent less fuel intense), reflecting the adoption of cleaner vehicles, whereas fuel intensity of retained vehicles increases by 0.21 liters per 100 kilometers (3.9 percent more fuel intense), which implies that treated household retain dirter vehicles. These results imply that while the subsidy substantially reduces gasoline demand from new vehicles, increased use of retained vehicles and retaining dirtier vehicles largely offsets these gains, limiting the overall reduction in household gasoline consumption.

Figure 8 presents visual evidence of the regression discontinuity design using the

calendar-year comparison. Each panel plots binned means of the outcome variable against weeks from the January 2020 policy change, with separate linear fits on either side of the discontinuity threshold (indicated by the vertical dashed line at zero). The figure is organized in three rows corresponding to the three panels of Table 1: Panel A displays gasoline demand in liters, Panel B shows driving behavior in kilometers, and Panel C presents fuel intensity in liters per 100 kilometers. Within each row, the left column shows total household outcomes, the middle column shows outcomes for new vehicles, and the right column shows outcomes for retained vehicles.

The graphical evidence corroborates the regression results. For newly leased vehicles (middle column), there are clear negative discontinuities in both gasoline demand and kilometers driven, along with a reduction in fuel intensity. For retained vehicles (right column), positive discontinuities are visible in both gasoline demand and kilometers driven, while fuel intensity remains relatively stable across the threshold. The total household effects (left column) show muted discontinuities, reflecting the offsetting behavior between new and retained vehicles.

Our main specification classifies all vehicles leased within the bandwidth period as "new vehicles." In Table C1, we provide a robustness check that separately examines vehicles leased within the bandwidth period versus those newly registered after the bandwidth period (post-July 2020). The results indicate no significant differences in vehicle adoption patterns outside the bandwidth period, confirming that all main treatment effects derive from vehicles leased within the bandwidth around the policy discontinuity.

Table 1: Gasoline demand using calendar-year comparison

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Gasoline Demand (liter)			
Post-subsidy lease renewal	-12.70	-84.50***	71.80***
	(27.93)	(15.66)	(27.70)
Mean (t-1)	1041.56	0	1041.56
B. Driving behavior (km/year)			
Post-subsidy lease renewal	277	-534**	811*
	(443)	(251)	(457)
Mean (t-1)	19822	0	19822
C. Fuel intensity (liter/100km)			
Post-subsidy lease renewal	-.16**	-.39***	.21**
	(.07)	(.06)	(.08)
Mean (t-1)	5.33	5.1	1.04

Notes: This table reports the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. The mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household$\times$year.

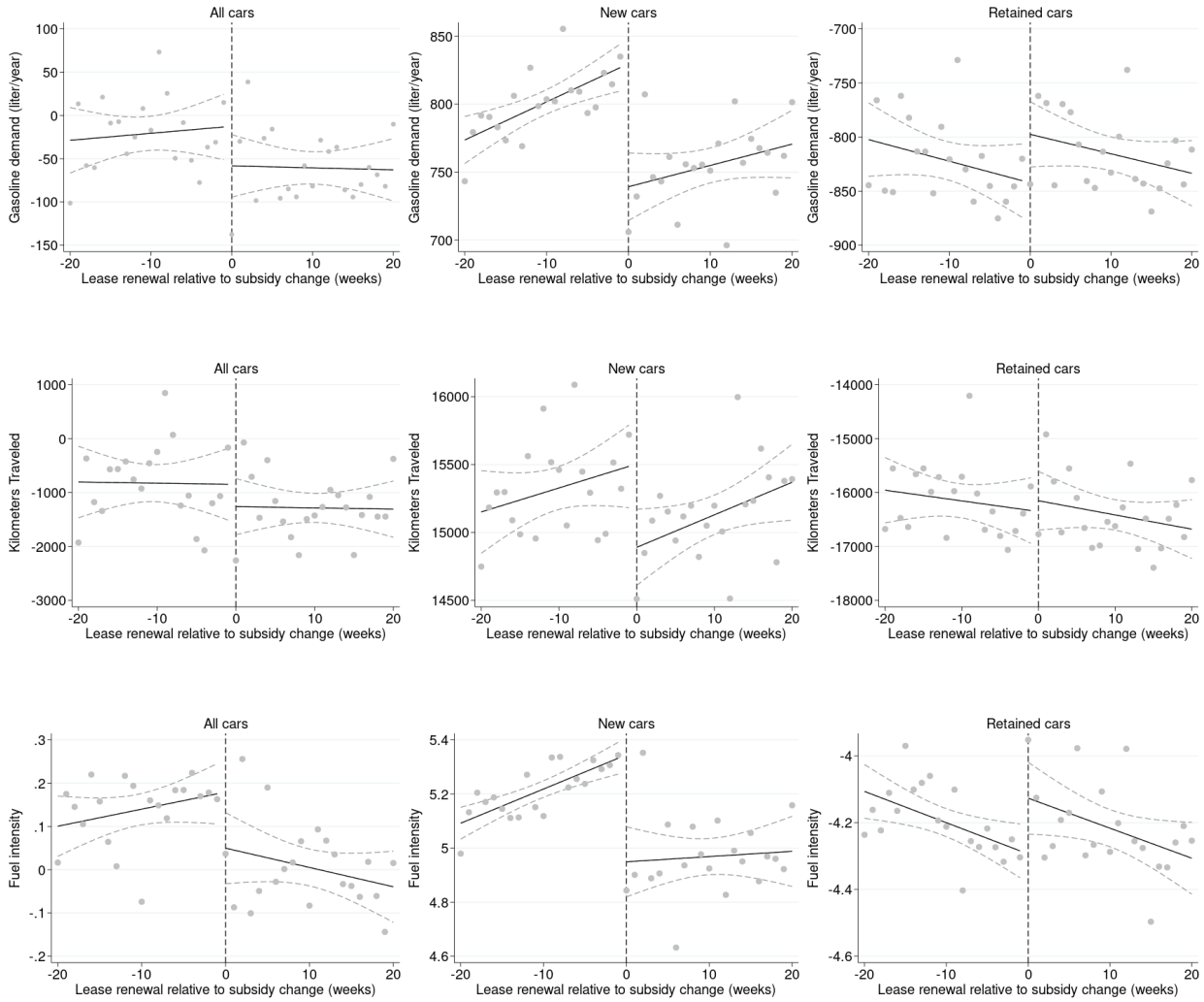


Figure 8: RD figures

Notes: This figure plots the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We use triangular kernel weights. The unit of observation is household×year.

Figure 9 provides a graphical decomposition of the estimated change in gasoline consumption from driving behavior and fuel intensity, separately for all vehicles (top panel), newly leased vehicles (middle panel), and retained vehicles (bottom panel). Following the decomposition of gasoline demand in equation (D5), each component represents the product of the estimated regression discontinuity coefficient and the corresponding pretreatment mean. The total effect on gasoline consumption (dark gray bars) is decom-

posed into changes attributable to driving behavior (red bars) and fuel intensity (light gray bars).

For newly leased vehicles, the reduction in gasoline consumption of approximately 83 liters per year is driven roughly equally by two mechanisms: lower fuel intensity—reflecting substitution toward cleaner vehicles such as PHEVs and BEVs—and reduced vehicle kilometers traveled. The fuel intensity effect captures the adoption of more fuel-efficient vehicles, while the driving behavior effect reflects reduced usage of these newly leased vehicles.

In contrast, retained vehicles exhibit an increase in gasoline consumption of approximately 86 liters per year, driven almost entirely by increased driving behavior, with negligible change in fuel intensity. This pattern is consistent with households reallocating kilometers from newly adopted PHEVs to their existing fossil-fuel vehicles. The fuel intensity of retained vehicles remains essentially unchanged, as these are existing cars in the household fleet whose technological characteristics are unaffected by the subsidy.

At the household level (top panel), these opposing forces—reduced gasoline consumption from new vehicles and increased consumption from retained vehicles—largely offset each other, yielding only a modest net increase in total gasoline consumption. The decomposition illustrates that while the subsidy successfully induces adoption of cleaner vehicles and reduces their fuel intensity, substitution in driving patterns across the household vehicle portfolio substantially erodes the potential gasoline savings. This finding highlights the importance of accounting for multi-vehicle household responses when evaluating the effectiveness of vehicle subsidy policies.

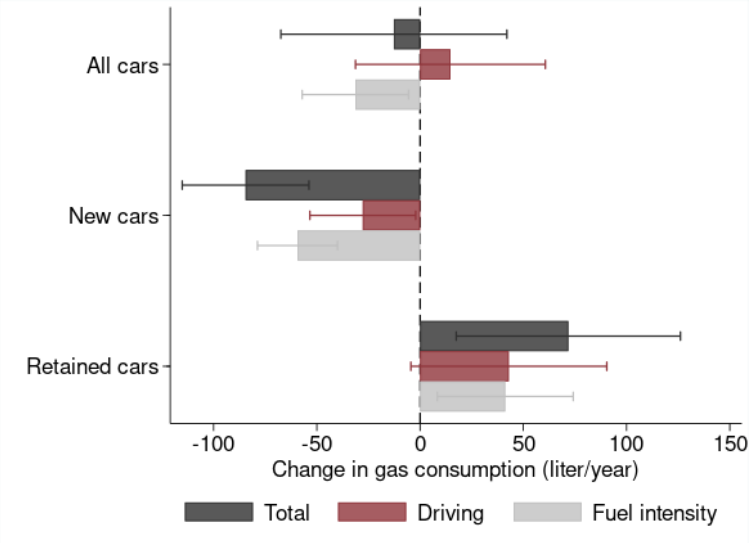


Figure 9: Decomposition of gasoline demand changes

Notes: This figure decomposes the estimated change in household gasoline consumption following the January 2020 subsidy increase into components attributable to driving behavior (red bars) and fuel intensity (light gray bars), with total effects shown in dark gray bars. Each component is calculated as the product of the estimated RD coefficient and the pre-treatment mean, following the decomposition in equation (D5). The top panel shows total household effects across all vehicles, the middle panel shows effects for newly leased vehicles only, and the bottom panel shows effects for retained vehicles only. Horizontal lines represent 95 percent confidence intervals constructed using robust standard errors. All outcomes are measured in liters per year using the calendar-year comparison.

B. Financial cost of feebate system

To implement the marginal fiscal cost changes implied by the January 2020 reform, our framework requires estimating the effect of the policy on the average subsidy received per new vehicle. Figure 10 illustrates the average subsidy per new car among households at lease renewal,

The policy shows a clear discontinuity in the average subsidy at the policy cutoff and we estimate that the reform increased the average subsidy per new vehicle by approximately 2,066 SEK ($\approx$ \$217.3). Relative to a pre-reform baseline of roughly 1,161 SEK ($\approx$ \$1221) in November 2019, This represents an 78 percent increase in the average subsidy payment. This effect captures the total fiscal impact of the reform on new vehicle subsidies, reflecting both the expansion of subsidy eligibility to a broader set of vehicles (the extensive margin) and higher subsidy amounts for eligible vehicles under the new policy regime (the intensive margin). This estimate represents the key fiscal cost parameter we use to evaluate the cost-effectiveness of the PHEV subsidy per ton of abated CO_2 emission.

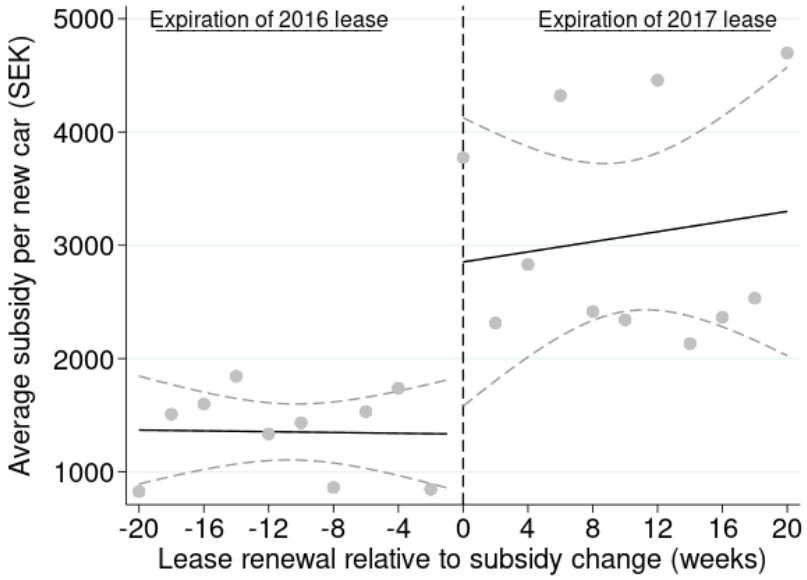


Figure 10: Average subsidy per new car

Notes: The figure illustrates the average subsidy per new car among households at the three year lease renewal. The data is grouped into biweekly intervals surrounding the 2020 policy change. The dashed vertical lines indicate the subsidy adjustment implemented in January 2020, with the time of the policy change normalized to zero.

C. Robustness

We assess the robustness of our main results through several alternative specifications that address potential threats to identification and alternative methods of controlling for confounding factors.

No "donut" specification. As a robustness check, we test whether our results are sensitive to the exclusion of observations near the cutoff. While our main specification excludes households with lease renewals near the policy cutoff in January 2020 to address potential manipulation concerns, we find that including these near-cutoff observations yields qualitatively similar results. Table C3 presents estimates without the donut restriction. Although the sample of compliers increases slightly and point estimates are somewhat smaller in magnitude, the overall pattern of results remains unchanged: subsidized PHEV adoption significantly increased gasoline consumption from retained vehicles.

Lease-timing controls. Our baseline specification compares gasoline consumption in the same calendar years (2018 and 2021) across treated and control households. This approach addresses the concern that treated and control households may differ slightly in the duration they retain their vehicles, as lease renewal dates vary across groups. While

controlling for lease-year-month fixed effects may ensure that any differences in vehicle age or replacement timing do not confound our estimates (Table C4), we find no meaningful difference in our main results using the RD design with covariates (Calonico et al. 2019).

Bandwidth choice. In addition to assessing the robustness of our results to different identification strategies, we further examine the sensitivity of our key findings to the length of the time window (i.e., the number of weeks included before and after the reform). We first examine the robustness of our main results to the choice of window length. A window that is too short may fail to capture meaningful shifts in vehicle choices following the policy change and limits the number of comparable lease renewal periods. Conversely, a window that is too long may smooth over changes in vehicle supply, which can introduce shifts in market shares. Figure C2 reports the regression discontinuity estimates for gasoline demand (Panel A), vehicle kilometers traveled (Panel B), and fuel intensity (Panel C) as the window length varies from 8 to 40 weeks. Each panel displays the point estimates for newly leased vehicles and retained vehicles, along with their corresponding 95 percent confidence intervals. With the exception of the narrowest windows, the estimated effects are remarkably stable across window lengths. In particular, the estimates converge quickly and remain nearly identical for window lengths of approximately 20 weeks and above, with larger windows yielding more precise estimates. This stability indicates that our results are not sensitive to the specific choice of bandwidth and supports the robustness of our main findings.

The consistency of results across these alternative specifications strengthens the causal interpretation of our estimates and supports the conclusion that subsidy-induced PHEV adoption generates only modest reductions in household gasoline consumption once portfolio-level driving adjustments are accounted for.

V. Direct Cost-Effectiveness of EV Subsidies

To estimate how expensive the feebate subsidy for plug-in hybrid electric vehicles (PHEVs) is per ton of abated CO_2 emissions, we compare the additional fiscal cost of the policy to the discounted lifetime reduction in gasoline consumption induced by the subsidy. On the cost side, we measure the policy's fiscal impact as the change in the average subsidy per new vehicle ($\Delta\gamma^{\text{subsidy}}$), which captures both the expansion in subsidy eligibility to a broader set of vehicles and the increase in subsidy amounts for eligible vehicles under the new policy regime. Appendix D.1 provides a derivation of how we compute the marginal cost of the subsidy.

On the emissions side, net emissions reductions arise from the subsidy-induced change in lifetime gasoline consumption, offset by additional emissions from electricity used to charge electric vehicles. We measure gasoline displacement directly using household-level data and decompose it into changes from newly adopted vehicles and portfolio-level driving adjustments across retained vehicles, and we account for emissions from EV charging under alternative assumptions about the marginal electricity generation mix. This framework allows us to translate our causal estimates of gasoline displacement into a measure of the marginal cost of carbon abatement implied by the policy.

Formally, we define the discounted subsidy-induced cost per unit of avoided emissions as:

$$CTA^{\text{subsidy}} = \sum_{h=1}^{H} \frac{\Delta\gamma^{\text{subsidy}}}{\frac{1}{(1+r)^h}\big(\underbrace{-\alpha \cdot \Delta\pi^{\text{gasoline}}_{comb.}}_{\Delta CO_2 \text{ from driving}} - \underbrace{\Delta KM^e \cdot \vartheta \cdot e \cdot CI^{grid}}_{\Delta CO_2 \text{ from charging}}\big) \underbrace{\cdot\varepsilon}_{\text{Marginal adopter}}}, \tag{5}$$

where r denotes the annual discount rate, and h indexes the vehicle's lifespan in years. The term $\Delta\pi^{gasoline}$ represents the reduction in annual gasoline demand (measured in liters) resulting from the PHEV subsidy change. The coefficient $\alpha \approx 2.3$ converts gasoline volume (liters) into the equivalent mass of CO_2 emissions (kilograms) based on the carbon content of gasoline.

Emissions from electric driving depend on the energy efficiency of the electric drivetrain, denoted by e, the share of vehicle kilometers driven on electricity, ϑ, and the carbon intensity of electricity generation, CI^{grid}, which varies with the marginal generation technology (e.g., renewables, natural gas, or coal). Finally, ε captures the share of marginal adopters induced by the subsidy and scales the emissions impacts to reflect the behavior of households whose vehicle choices are affected by the policy.

To quantify the social cost of carbon emissions associated with the policy, as formalized in equation (5), we combine our estimated policy elasticities for subsidy costs and gasoline consumption with parameter values drawn from the Swedish vehicle registry. Using data on retired vehicles, we estimate the average vehicle lifespan in Sweden to be approximately 18 years. Future gasoline savings are discounted to present value using an annual discount rate of r = 5 percent. Table 2 summarizes the parameter values employed in the abatement cost calculations.

Table 2: Parameter estimates used to calculate abatement costs

Coefficient	Description	Period	Value	Source
Panel A: Registry Data				
h	Vehicle lifespan index (in years)	2005-2023	18	Vehicle registry
e	Energy efficiency of electric engine	2019	.25	Vehicle registry
ϑ	Share of driving on electric drivetrain	2019	.3	Vehicle registry
Panel B: Policy Coefficients				
$\Delta\pi_{comb.}^{\text{gasoline}}$	Change in annual gasoline demand (liters)	2018/2021	-12.7	Table 1
$\Delta\gamma^{\text{subsidy}}$	Change in the average subsidy per new car	2020	217.3	Figure 10
ΔKM^e	Change in electric KMs	2018/2021	528	Table C2
ε	Share of marginal adopter	2020	.6	
Panel C: Parameter assumptions				
r	Annual discount rate		.05	
CI^{grid}	Carbon intensity of grid (renewables, gas, or coal)		(0, .25, .45)	

Under the assumption that electric vehicle charging does not generate additional carbon emissions – corresponding to a scenario in which renewable generation is on the margin – the implied cost per ton of carbon abated (CTA) under the renewables scenario can be calculated as follows:

$$CTA^{renewable} = \sum_{h=1}^{18} \frac{\$217.3}{\frac{1}{(1+0.05)^h} \cdot \left(2.3 \frac{\text{kg CO}_2}{\text{l}} \cdot 12.7 \frac{\text{l}}{\text{year}} \cdot \frac{1\,\text{t}}{1000\,\text{kg}}\right) \cdot .6}$$

$$= \frac{\$217.3}{.205\,\text{t CO}_2} \approx \$1,061 \text{ per ton CO}_2$$

The estimated cost of carbon abatement implied by the policy is approximately $1,061 per ton of CO_2. Relative to a benchmark social cost of carbon of $126 per ton, this implies that the policy would require a vehicle lifespan of 151 years to break even on climate benefits alone—more than four times the average vehicle lifespan of 18 years.

Even when evaluated using the upper bound of the 95 percent confidence interval for gasoline reduction (67 liters per year), which translates to 1.09 metric tons of CO_2 abated over the discounted vehicle lifespan, the policy cost per ton remains $199.4. This estimate marginally exceeds the social cost of carbon benchmark of $126 per ton in Sweden and is also above the most recent Environmental Protection Agency estimate of the social cost of carbon of $190 per ton, suggesting the policy is not even cost-effective even under the

most optimistic scenario for emissions reductions. Under dirtier electricity mixes—where the marginal emissions from charging PHEVs are larger than zero—the climate benefits would be further diminished, making the policy even less cost-ineffective. Thus, even the most favorable interpretation of our results indicates that PHEV subsidies fail to deliver cost-effective climate benefits. We further back out the confidence with which we can reject a cost of CO_2 abatement below \$200 per ton and find that this threshold is rejected at exactly the 5 percent significance level.

We can also evaluate the policy using only the gasoline reduction from new vehicles, excluding the offsetting effect of retained fossil fuel vehicles. As documented in Table 1, the gasoline demand reduction from new cars alone equals 84.5 liters per year, which translates to 1.36 metric tons of CO_2 abated over an 18-year discounted vehicle lifespan. This yields a cost per ton of \$159—nearly cost-effective relative to the social cost of carbon of \$126 per ton. This is close to Cole et al. (2023), who estimate costs of approximately \$95 per ton of CO_2 abated from the Inflation Reduction Act of 2022 (IRA). However, when we account for this retention effect, the net emissions reduction falls substantially, pushing the cost per ton to \$1,061—more than four times the social cost of carbon. This comparison underscores an important methodological point: evaluating vehicle subsidies without accounting for their effect on the existing vehicle fleet can yield systematically biased and overly optimistic estimates of cost-effectiveness.

We further assess the sensitivity of these estimates to assumptions about the marginal electricity generation mix. Using a carbon intensity of 0.45 tCO_2/MWh for natural gas–fired generation (U.S. Environmental Protection Agency 2023) and 0.91 tCO_2/MWh for coal-fired generation (International Energy Agency 2023) and subtracting the additional emissions from EV charging, the implied cost of abatement rises to \$1,808 per ton under a natural gas scenario and becomes negative under a coal scenario. These values substantially exceed standard estimates of the social cost of carbon, indicating that, once charging-related emissions are taken into account, the subsidy is unlikely to be cost-effective on emissions grounds alone.

VI. Concluding Remarks

This paper provides causal evidence on how households adjust across an entire portfolio of durable goods in response to subsidy-based climate policy. Exploiting a policy-induced increase in Swedish PHEV subsidies and predetermined lease renewal timing, we show that while subsidies shift new vehicle choice toward cleaner models, households simultaneously reallocate driving toward retained gasoline vehicles. The net effect on gasoline

consumption is small and statistically indistinguishable from zero.

For regulators, the central implication is that technology subsidies do not replicate the incentive structure of Pigouvian pricing. Because EV subsidies lower the cost of acquiring clean vehicles without raising the marginal cost of driving dirty ones, households can substitute across assets in ways that attenuate emissions reductions. Adoption rates of clean technology therefore provide an incomplete and potentially misleading measure of policy performance.

For researchers and modelers of the energy transition, the results underscore the importance of embedding empirically disciplined substitution patterns into quantitative frameworks. Policies that target discrete technologies operate in sub-optimal environments where unpriced margins remain active. More generally, in any setting where agents allocate effort or utilization across multiple assets – e.g. housing, agricultural inputs, education, or health investments –interventions that fail to price the underlying externality may reshuffle costs rather than reducing them. The contrast between subsidy-based policy and Pigouvian pricing is not merely conceptual; it has first-order implications for projected emissions and welfare.

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Appendix

Clean Cars, Dirty Margins: Portfolio Responses to Electric Vehicle Subsidies

Teevrat Garg *David Rapson* *Sebastian Tebbe*

A. Additional Summary Statistics

Table A1: Sample characteristics

	Our sample		New car adopter		Average person	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
A. Demographic characteristic						
Age	49.74	13.41	51.27	14.98	47.87	18.70
Female	0.45	0.50	0.35	0.48	0.50	0.50
Salary (SEK)	402,713	269,013	372,172	499,684	234,225	280,772
Self-employed	0.06	0.23	0.11	0.31	0.07	0.25
Retired	0.13	0.33	0.21	0.40	0.21	0.40
Married	0.71	0.44	0.71	0.45	0.55	0.49
Children	0.39	0.48	0.34	0.47	0.43	0.48
Years of education	12.92	2.31	12.73	2.48	12.23	2.62
B. Vehicle characteristic						
Number of cars	1.12	0.33	1.39	2.44	0.58	1.43
Vehicle kilometers traveled	13,065	5,851	10,994	25,018	6,549	12,485
Carbon intensity (gCO2/km)	118	24.47	118	44.41	139	53.42
Fuel intensity (l/100km)	5.09	1.06	5.27	1.99	6.40	1.87
Vehicle weight (kg)	1,851	238	1,986	297	1,954	292
Gasoline demand (l)	673	358	593	1,474	389	755
N(Observation)	18,954		273,365		7,866,447	

Notes: The table presents summary statistics for socio-demographic characteristics (Panel A) and vehicle attributes (Panel B) across three groups: households in our study sample, new vehicle adopters, and the population of vehicle owners in Sweden.

Table A2: Car characteristics by treatment group

	Treated car		Counterfactual car		New car	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
A. Car characteristic						
Fuel intensity (l/100km)	5.22	1.02	5.41	1.04	5.54	1.85
Gasoline demand (l)	946.46	630.55	851.11	1774.86	737.08	634.58
Vehicle kilometers traveled	18026.75	10337.87	15734.03	30193.77	13744.54	11219.57
N(Observation)	3445		5164		164250	

Notes: This table reports the average characteristics of newly registered vehicles for three groups: households whose leases expired in 2020 (treated group), households with lease expiration in 2019 (counterfactual group), and the full population of newly registered vehicles during the corresponding period (new car).

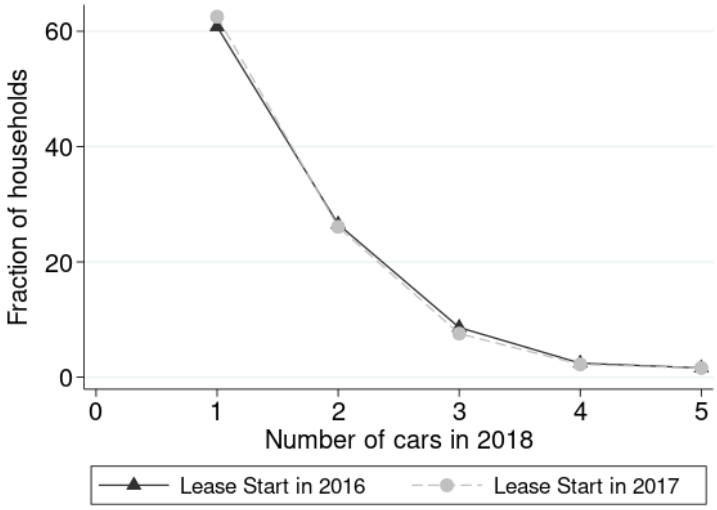


Figure A1: Distribution of number of vehicles

Notes: This figure presents the pre-treatment distribution of the number of vehicles owned by treated (dark grey) and control (light grey) households.

B. Estimation of Vehicle Usage Data

B.1. Swedish odometer readings

The annual vehicle kilometers traveled data in the register is derived from odometer readings collected by the Swedish Transport Agency (*Transportstyrelsen*) during mandatory vehicle inspections and periods of deregistration. To calculate annual vehicle usage, at least one inspection must have taken place between January 1st of the reference year and January 31st of the following year. Inspection data is matched to the Road Traffic Register (*Vägtrafikregistret*) using registration numbers. The odometer readings from inspections are then paired, forming *odometer pairs*, which consist of two consecutive readings, with at least one recorded after January 1st of the reference year. An odometer reading is typically recorded during a regular inspection but can also originate from the registration date, where vehicle kilometers are set to zero.

A vehicle may have multiple odometer pairs in a given year if it has been inspected at least twice during that period. If no prior inspection exists, an odometer pair is created using the first registration date as the initial inspection date and an odometer reading of zero. For a detailed description, see *Mileage Database.*

Using odometer pairs, the annual vehicle kilometers traveled (KM) are estimated as:

$$\text{KM} = \underbrace{\left(\frac{\text{Odometer}_{t+1} - \text{Odometer}_{t}}{\text{Operational Days}}\right)}_{\text{Daily Usage}} \times \text{Days}. \tag{B1}$$

where $Odometer_{t+1}$ and $Odometer_t$ refer to the odometer reading at the second and first inspection, $Days$ are the number of days the vehicle was in operation between inspections. The number of operational days are calculated as the total number of days between inspections minus any periods when the vehicle was deregistered. To determine the total usage for a given year, the daily mileage is multiplied by the number of days within that year.

For vehicles without an inspection during the reference year, and thus lacking a valid odometer pair, vehicle usage is estimated using similar vehicle models with vehicle usage data and that have been inspected. Between 25 percent of reported mileage data for a given reference year is based on estimates. Newly registered vehicles typically do not require inspection until three years after registration for passenger cars.

B.2. PHEV utility factor adjustment

For PHEVs, the share of kilometers driven on the electric drivetrain is not directly observed in administrative data. Instead, manufacturers report an official *Utility Factor* (UF)

under the Worldwide Harmonized Light Vehicles Test Procedure (WLTP), which maps a vehicle's certified charge-depleting range to the fraction of driving assumed to occur in electric mode. These WLTP utility factors are used for regulatory compliance and emissions certification and are based on standardized test-cycle assumptions.

Formally, the WLTP utility factor is defined as

$$UF(R_{\text{CDC}}, d_n) = 1 - \exp\left[-\sum_{i=1}^{10} c_i \left(\frac{R_{\text{CDC}}}{d_n}\right)^i\right], \tag{B2}$$

where R_{CDC} denotes the WLTP-certified charge-depleting range in kilometers, $d_n = 800$ km is the normalization parameter specified in European regulation, and $\{c_i\}_{i=1}^{10}$ are fixed coefficients defined by the regulatory framework.

While these WLTP-based utility factors represent official manufacturer-reported values, a growing body of evidence shows that they overstate real-world electric driving for PHEVs. In practice, PHEV users tend to drive a smaller share of kilometers in electric mode than implied by WLTP test cycles, due to charging constraints, trip patterns, and user behavior.

To account for this discrepancy, we adjust the WLTP utility factors following the approach proposed by the International Council on Clean Transportation (ICCT). Specifically, we preserve the regulatory functional form of the utility factor but replace the WLTP normalization parameter with an empirically calibrated value, $d_n^{\text{adj}} = 4{,}260$ km, which reflects observed PHEV driving behavior in real-world data.

Figure B1 illustrates how this ICCT adjustment reduces the implied share of electric driving relative to the WLTP-based utility factor, particularly for PHEVs with longer certified electric ranges. Throughout the analysis, we use these ICCT-adjusted utility factors to allocate PHEV vehicle kilometers traveled between electric and gasoline driving. This approach ensures that our fuel-use and emissions calculations are grounded in observed usage patterns rather than regulatory test-cycle assumptions.

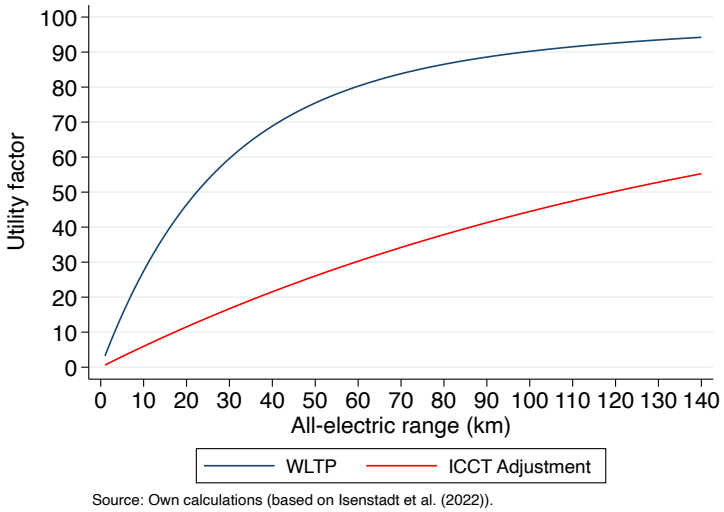


Figure B1: PHEV usage by range

Notes: WLTP refers to the Worldwide Harmonized Light Vehicles Test Procedure, providing the official driving range and utility factors. The ICCT adjustment reflects observed PHEV driving patterns, so we adjust the WLTP-based PHEV utility factors to align with actual usage.

C. Additional Figures and Tables

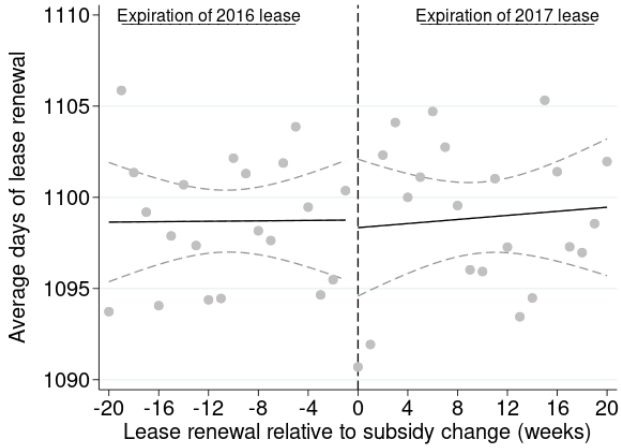


Figure C1: Number of days between second and first lease

Notes: The figure shows the average number of days between the lease renewal date and the date of the lease initiation for households who adopt a new vehicle at the lease renewal around the 2020 policy change. Dashed vertical lines denote the subsidy change of January 2020 (normalized to 0).

Table C1: Gasoline demand from renewal and non-renewal cars

	Vehicle type			
	(1) All cars	(2) Renewal	(3) Non-renewal	(4) Retained cars
A. Gasoline Demand (liter)				
Post-subsidy lease renewal	-12.70	-82.49***	-2.01	71.80**
	(27.93)	(14.43)	(7.09)	(27.70)
Control Pre-Mean (2018)	1042	0	0	1042
B. Driving behavior (km)				
Post-subsidy lease renewal	277.4	-476.5**	-30.2	811.1*
	(442.6)	(196.9)	(146.6)	(456.8)
Control Pre-Mean (2018)	19822	0	0	19822
C. Fuel intensity (liter/100km)				
Post-subsidy lease renewal	-0.16**	-0.38***	-0.07	0.21**
	(0.07)	(0.06)	(0.52)	(0.08)
Control Pre-Mean (2018)	5.3	.	.	5.3

Notes: This table reports the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) newly leased cars during the bandwidth period (3) new cars after the bandwidth period (July 2020), and (4) retained cars. Covariate-adjusted 95 percent confidence interval, robust p-values and the mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household×year.

Table C2: Effect on vehicle usage by fuel type

	Vehicle type		
	(1) Plug-in	(2) Fossil	(3) Hybrid
A. Vehicle Ownership			
Post-subsidy lease renewal	0.037***	0.029	-0.074
	(0.014)	(0.037)	(0.523)
Control Pre-Mean (2018)	0.004	1.447	–
B. Vehicle Kilometers			
Post-subsidy lease renewal	528.1***	-269.1	18.4
	(180.3)	(501.3)	(253.3)
Control Pre-Mean (2018)	50.9	18833.8	937.4

Notes: This table reports the RD estimates using calendar year comparisons for changes in annual vehicle kilometers traveled, separately by fuel types: (1) PHEVs and BEVs (2) petrol and diesel cars, and (3) hybrid and alternative-fuel cars. The mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household×year.

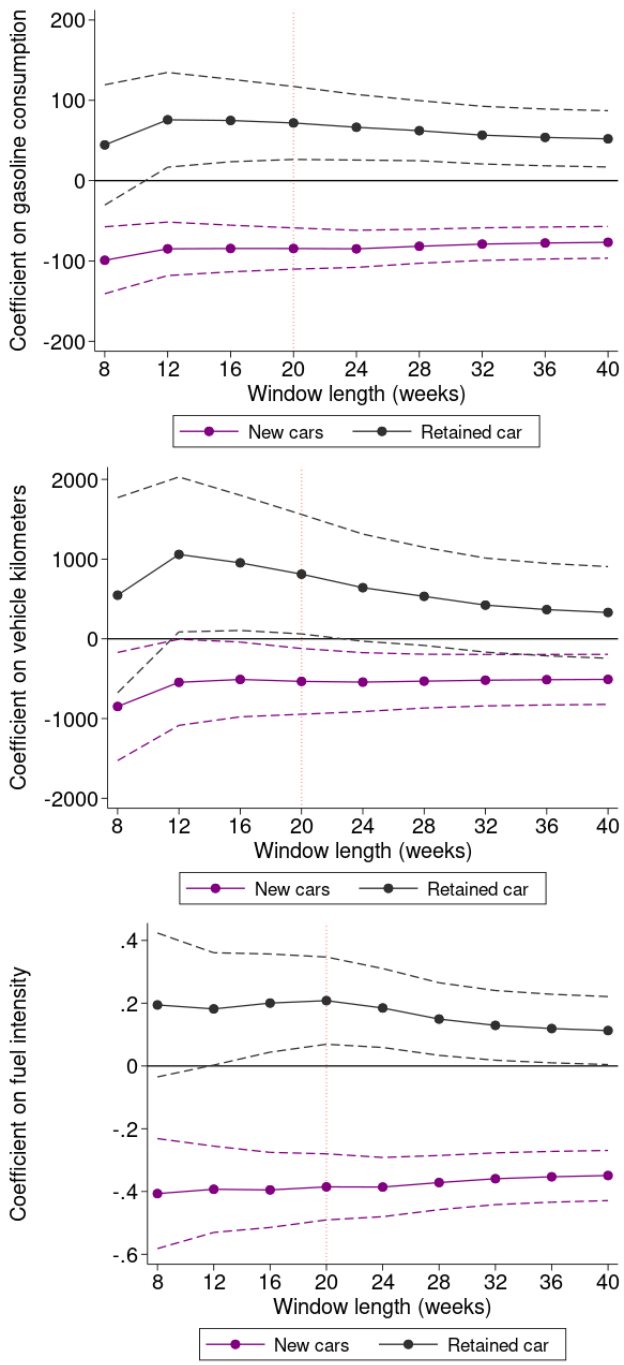


Figure C2: Coefficient estimates by window length

Notes: The figure show the regression discontinuity estimate on the gasoline demand (Panel A), kilometers travelled (Panel B), and fuel intensity (Panel C) for new (purple) and retained cars (black) bu window length. Red dashed vertical lines denote the current window.

Table C3: Gasoline demand using no donut restriction

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Gasoline Demand (liter)			
Post-subsidy lease renewal	12.67	-76.12***	88.79
	(54.88)	(29.55)	(54.18)
Mean (t-1)	1041.56	0	1041.56
B. Driving behavior (km/year)			
Post-subsidy lease renewal	589	-705	1294
	(865)	(478)	(874)
Mean (t-1)	19822	0	19822
C. Fuel intensity (liter/100km)			
Post-subsidy lease renewal	-.10	-.33***	.40***
	(.11)	(.11)	(.15)
Mean (t-1)	5.35	5.11	1.05

Notes: This table reports the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. Covariate-adjusted 95 percent confidence interval, robust p-values and the mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We use triangular kernel weights. The unit of observation is household$\times$year.

Table C4: Gasoline demand controlling for lease-timing controls

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Gasoline Demand (liter)			
Post-subsidy lease renewal	15.41	-67.31	82.72
Robust 95% CI	[-71 ; 95]	[-118 ; -25]	[1 ; 166]
Robust p-value	0.769	0.003	0.047
Control Pre-Mean (2018)	1042	0	1042
B. Driving behavior (km/year)			
Post-subsidy lease renewal	821.20	-305.69	811.07
Robust 95% CI	[-284 ; 2322]	[-1104 ; 402]	[-262 ; 2443]
Robust p-value	0.125	0.361	0.114
Control Pre-Mean (2018)	19822	0	19822
C. Fuel intensity (liter/100km)			
Post-subsidy lease renewal	-0.14	-0.33	0.18
Robust 95% CI	[-.397 ; -.006]	[-.551 ; -.171]	[-.093 ; .406]
Robust p-value	0.043	0.000	0.219
Control Pre-Mean (2018)	5.3	.	5.3

Notes: This table reports the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. Covariate-adjusted 95 percent confidence interval, robust p-values and the mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals two weeks before and after the policy change in January 2020. We control for Lease-Year Month fixed effects. We use triangular kernel weights. The unit of observation is household$\times$year.

D. Derivation of Policy Implications

D.1. Cost of policy

Let the total cost of the subsidy policy be:

$$C = J \cdot \bar{S} = M \cdot \gamma \cdot \bar{S} \tag{D1}$$

where J is the number of subsidized vehicles, M is the number of newly registered vehicles, $\gamma = \frac{J}{M}$ is the market share of subsidized vehicles, $\bar{S}$ is the average subsidy per subsidized vehicle. Both γ and $\bar{S}$ are functions of the subsidy S_j. In particular, γ is endogenous to S_j as more generous subsidies increase the adoption rate.

To analyze how the subsidy affects total cost per newly registered vehicle (divided by M), we compute the total derivative of cost per vehicle with respect to S_j:

$$\frac{1}{M} \cdot \frac{dC}{dS_j} = \frac{d}{dS_j}(\gamma \cdot \bar{S}) = \frac{d\gamma}{dS_j} \cdot \bar{S} + \gamma \cdot \frac{d\bar{S}}{dS_j} \tag{D2}$$

This per-vehicle cost change captures two key channels:

- $\frac{d\gamma}{dS_j} \cdot \bar{S}$: Eligibility expansion margin — An increase in the number of vehicles adopting the subsidy
- $\gamma \cdot \frac{d\bar{S}}{dS_j}$: Subsidy intensity margin — Increase in the average subsidy among adopters

The total cost of the policy increases due to two key effects: (i) a rise in the average subsidy among adopters, and (ii) an increase in the number of vehicles adopting the subsidy. This expression can be interpreted as the marginal increase in policy cost per newly registered vehicle due to a change in the subsidy S_j.

To express the cost per subsidized car (as opposed to per newly registered car), we scale the per-new-vehicle cost by the inverse of the adoption rate when evaluating marginal costs:

$$\left(\frac{d\gamma}{dS_j} \cdot \bar{S} + \gamma \cdot \frac{d\bar{S}}{dS_j}\right) \cdot \frac{M}{J} = \left(\frac{d\gamma}{dS_j} \cdot \bar{S} + \gamma \cdot \frac{d\bar{S}}{dS_j}\right) \cdot \frac{1}{\gamma} \tag{D3}$$

To evaluate cost changes per subsidized vehicle, we must adjust for the share of adopters in the new vehicle market.

D.2. Decomposition of gasoline consumption

A household's h total car-related gasoline demand G is equal to the fuel intensity (measured in l/100km) (F_i) multiplied by the annual vehicle kilometers traveled (KM_i), summed

over all cars i in a given year t. We partition this expression into two vehicle categories: (1) retained cars ($\mathcal{K}$), and (2) new cars ($\mathcal{E}$)

$$Gas_h = \sum_{i=1}^{N} I_i \cdot F_i \cdot KM_i = \underbrace{\sum_{k \in \mathcal{K}} I_k \cdot F_k \cdot KM_k}_{\text{(1) Retained cars}} + \underbrace{\sum_{e \in \mathcal{E}} I_e \cdot F_e \cdot KM_e}_{\text{(2) New entrant cars}} \tag{D4}$$

where $I_i \in \{0, 1\}$ indicates whether car i is in the vehicle portfolio.

To measure how the subsidy change τ affect a household's gasoline demand, we differentiate the gasoline demand with respect to the subsidy change τ as follows:

$$\frac{dGas}{d\tau} = \underbrace{\sum_{k \in \mathcal{K}} \left(F_k \frac{d\, KM_k}{d\tau} + KM_k \frac{d\, F_k}{d\tau} \right)}_{\text{(1) Driving in retained cars and fuel in exiting cars}} + \underbrace{\sum_{e \in \mathcal{E}} \left(F_e \frac{d\, KM_e}{d\tau} + KM_e \frac{d\, F_e}{d\tau} \right)}_{\text{(2) Driving and fuel of new cars}} \tag{D5}$$

This formulation decomposes the total change in household gasoline demand, $\frac{dG}{d\tau}$, into two behavioral margins—*vehicle intensity* and *driving behavior*—for each of the two vehicle categories: entrants, and retained vehicles.

- **(1) Retained cars** ($\mathcal{K}$): describe adjustments in *fuel efficiency* and *driving intensity* among continuing vehicles, corresponding to within-fleet behavioral or technological responses.
- **(2) New entrant cars** ($\mathcal{E}$): capture the effect of changes in *fuel intensity* ($dF_e/d\tau$) and *vehicle usage* ($dKM_e/d\tau$) among newly adopted vehicles entering the household fleet.

Hence, the total derivative $\frac{dG}{d\tau}$ captures four distinct margins—two (fuel intensity and driving behavior) for each of the two fleet categories—providing a decomposition of how household fuel demand responds to the feebate change. We estimate each policy coefficient empirically using our RD design.